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THE ANALYSIS OF INTERVENTION IMPACTS AND CHANGE IN CRIME: A TASK FORCE ANALYTICAL EXEMPLAR

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William M. Holmes, Ph.D.
Director

Teresa A. Mayors, M.A.
Evaluation Specialist

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Statistical Analysis Center
Massachusetts Committee on Criminal Justice
100 Cambridge Street, Room 2100
Boston, MA 02202

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ABSTRACT

This study discusses analytical procedures for examining the effect of drug enforcement task forces on change in crime. Using statewide, community level data, it compares changes in crime between task force and non-task force cities at the beginning stages of implementing the task forces. It identifies problems in working with crime change data when making such comparisons. It discusses strategies for dealing with these problems.

One purpose of the evaluation is to develop a model for the analysis of change data that deals with problems common to a variety of data when evaluating drug enforcement projects. A second purpose is to provide baseline information on differences between task force and non-task force communities.

The project uses a longitudinal, non-equivalent control group design. The use of non-equivalent control groups was dictated by the assignment of task force grants to communities that request and adequately justify participation in a task force. The longitudinal component of the design is necessary to provide measures of crime change in the task force and non-task force communities. The design is based on statistical models for the analysis of change. The combination of longitudinal and non-equivalent control group data is expected to compensate for some of the limitations of using either of these methods alone.

Three problems in the analysis of change are illustrated: correlation of change with baseline measures, trends in change, and correlation of change with confounding factors. These problems are reduced by introducing appropriate control variables in the analysis. These findings imply that evaluations of drug control programs using change measures as outcomes can produce spurious findings if these problems are not addressed by the design or the analysis. More reliable and valid results are produced when these problems are considered.

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With the advent of the Criminal Justice Statistics Association (CJSA) Consortium for Drug Strategy Impact Assessment, a major concern has been the development and use of common measures and analytical models for drug enforcement impacts. One area of particular concern is the analysis of data on drug enforcement efforts of multijurisdictional task forces. Within Massachusetts a similar concern is raised concerning single jurisdiction task forces, referred to hereafter as "Target Cities." How does one analyze comparable data for multijurisdictional drug task forces, referred to in the state as "Community Impact" projects, and for Target Cities to examine whether these drug enforcement projects had any effect? How, especially, using the common measures of the CJSA Drug Consortium project?

The most obvious source of an analytical model is to look at the statements of intended impacts within the subgrant applications by the task forces. Massachusetts requires all Drug Enforcement subgrant applications to contain a statement of goals and objectives for each project and the intended impacts. These impacts are supposed to be formulated in concrete, empirical terms. For example, all the task forces include a stated impact of "increasing drug arrests." Many also have a stated impact of "reducing violent crime." These expected impacts lead to basic questions. How does one measure "increases in drug arrests" or "reductions in violent crime"? Also, how does one analyze such measures to determine whether such changes have occurred?

The measurement question has been addressed by numerous discussions of the CJSA Drug Consortium. Some, partial, answers have resulted from these discussions. A common set of measures to be collected for drug task force activity has been agreed upon. Although, many states collect additional, alternative information as well. Every state in the Consortium has agreed to collect quarterly information on arrests for drug trafficking and drug possession for the jurisdictions in their task forces. This information also meets requirements by BJA for these data as part of a state's drug strategy.

Such data are usually collected by quarterly reports directly from the police departments involved, rather than relying upon UCR arrest statistics (commonly referred to as "ASR" data). The former process is more timely and may allow breaking down the figures by whether the arrest resulted from the drug grant or regular department enforcement. Nevertheless, for some analytical purposes the UCR arrest statistics may be adequate.

ANALYSIS OF CHANGE

The two impacts mentioned above both focus on expected changes: changes in arrests or changes in violent crime offenses. This suggests that procedures for the analysis of change may be applicable to models of drug enforcement impacts. Several issues raised by the literature on analysis of change apply to this situation. This paper illustrates three of those issues and models for dealing with them: correlation of change measures with

their baseline values, correlation of current change with prior change, and association of change measures with alternative confounding factors.

Correlation Between Change and Baseline

The problem of correlation of a change measure with its baseline value has been known for some time (Kessler, 1977). Such a correlation creates ambiguity as to whether the indicator measures change or simply the baseline level of crime. Perhaps large increases in arrests or decreases in violent crime only occur in jurisdictions having large crime rates to begin with. In such a case, the observed change may represent high crime rates, rather than true increase or decrease.

The traditional strategy for dealing with a correlation between a change measure and its baseline is to control for the baseline value (Kessler, 1977). Statistically, this means using the baseline level of crime as a control variable in the analysis. This removes the problem if the correlation with the baseline value is not too extreme. Regression analysis and Analysis of Covariance (ANCOVA) are both used to control for the baseline value.

This strategy, however, only produces reliable results if the correlation is not too extreme. With extreme correlation between baseline and change measures, controlling for the baseline value produces unreliable results; but not controlling for the baseline produces false conclusions about change. In such a circumstance, the design may be incapable of answering questions about change outcomes. The impact on change becomes totally confounded with differences in the baseline situation.

There are no agreed upon standards for determining when the correlation is too extreme. It is not unwise, however, to be skepticle when the correlation exceeds .70 and controlling for the baseline value produces radically different findings.

When evaluation projects show such a result, it means that the findings regarding impacts on change are not reliable--even though simple descriptive findings are accurate. Subsequent evaluations of change measures having this problem may require using experimental designs with random assignment to intervention and control groups, since this will remove differences in baseline values (Cook and Campbell, 1979; Lempert and Visher, 1988).

Trends in Change

It has long been known that trends in the data often produce correlation between change measures at two points in time (Davidson, 1972). The observed change may be an artifact of prior trends. Systematic error in the crime measures across time may also magnify these trends.

Interrupted time series analysis was developed, in part, to deal with this problem. However, with only a small number of data points across time such a method is not feasible. Regression analysis or analysis of covariance are alternative approaches to control for prior change if there are multiple units of observation (a number of towns, say, who receive the same type of grant).

Confounding Factors

The problem of association of change measures with alternative confounding factors is essentially a problem of misspecification, the failure to include and control for important influences (Kessler, 1977; Lempert, 1966). For example, changes in unemployment rates or population migration can affect change in crime independently of drug enforcement interventions. Larger cities may also be qualitatively different from smaller towns with respect to changes in their crime rates. In addition, cities will vary in the extent to or speed with which projects are implemented.

This problem can be addressed by adding key control variables to one's analysis. The difficulty is determining which key control variables to add. Research literature and qualitative interviews with criminal justice officials can suggest some of these confounding factors. Such things as city size, density, unemployment, divorce, age structure of the population, poverty, and geographic migration are factors that might be considered as possible key variables. Proximity to major transportation routes can also correlate with rate of change. Even gross measures of project implementation may prove useful when comparing sites that have very complete versus little or no implementation.

METHODOLOGY

These problems and analytical models for dealing with them are illustrated using UCR violent crime offense statistics for 242 police jurisdictions (cities and towns) in Massachusetts between the years 1986 to 1988. Data for this analysis were provided by the Crime Reporting Unit of the Criminal History Systems Board of Massachusetts. The Crime Reporting Unit is responsible for collecting, cleaning, and sending to the FBI the regular UCR statistics for the Commonwealth.

Dichotomous variables were coded indicating whether each town or city was or was not a member of a drug enforcement task force (Community Impact Project) or a recipient of a Target City grant. Yearly changes in UCR crimes were examined for number of offenses and for rate per 100,000 population.

Differences between task force cities (Community Impact cities and Target Cities) and non-task force cities were examined. One-way Analysis of variance compared mean crimes and crime rates for the two groups. Control variables were then introduced using Analysis of Covariance (ANCOVA) to remove some of the problems mentioned above.

FINDINGS

Table 1 compares task forces and target cities with other cities on changes in crime using analysis of variance F-tests. These simple comparisons suggest (erroneously, as will be shown) not only that task forces had no effect on changes in crime, but also that receiving target city grants exacerbates violent crime in the target cities.

TABLE 1

CRIME CHANGES FOR TASK FORCE CITIES

CRIME	TASK FORCE MEAN	N	NOT TASK FORCE MEAN	N	F	DF	P
*Change in Total Crime Index	-114.43	116	119.23	126	0.9	1,240	.35
*Change in Property Crime Index	-121.45	116	66.25	126	0.9	1,240	.35
*Change in Violent Crime Index	.95	116	31.92	126	1.6	1,240	.21
*Change in Violent Crime Rate	- 12.64	108	33.40	103	0.8	1,209	.38
*Change in Property Crime Rate	- 66.28	108	85.40	103	0.6	1,209	.45
*Change in Total Crime Rate	- 47.12	108	144.55	103	0.6	1,209	.45
**Change in Violent Crime Rate	33.48	110	21.49	123	0.3	1,231	.59
**Change in Property Crime Rate	168.30	110	- 66.79	123	1.4	1,231	.24
**Change in Total Crime Rate	168.47	110	- 47.20	123	0.7	1,231	.40

CRIME CHANGES FOR TARGET CITIES

CRIME	TARGET CITY MEAN	N	NOT TARGET CITY MEAN	N	F	DF	P
*Change in Total Crime Index	692.65	7	-13.19	235	0.9	1,240	.34
*Change in Property Crime Index	304.83	7	-33.51	235	0.3	1,240	.57
*Change in Violent Crime Index	200.24	7	11.62	235	6.8	1,240	.01
*Change in Violent Crime Rate	200.24	7	16.69	204	8.1	1,209	.00
*Change in Property Crime Rate	304.83	7	- 2.43	204	0.3	1,209	.58
*Change in Total Crime Rate	692.65	7	24.27	204	0.9	1,209	.45
**Change in Violent Crime Rate	14.81	6	27.48	227	0.1	1,231	.86
**Change in Property Crime Rate	253.38	6	38.67	227	0.1	1.231	.73
**Change in Total Crime Rate	268.19	6	48.98	227	0.1	1,231	.78

*Changes are for 1988-87
**Changes are for 1987-86

The lack of a difference between task force cities and non-task force cities is not totally surprising. The startup years for these cities were mostly in 1987 and 1988. Some towns did not even join task forces until 1989. For many cities and towns the projects were not yet sufficiently implemented to show what effect the task force participation might have on measures of impact. When 1989 data become available and are merged with this database, more of a difference is expected.

The results in Table 1 are potentially confounded by the three problems mentioned above. The difference between Target and non-Target cities may be an artifact of these problems. Change of violent crime in Target Cities may be larger because those cities start off with a higher baseline level of violent crime (one of the reasons why those cities were selected as Target Cities). The change in violent crime may also be an artifact of a prior trend in increasing crime that existed before

those cities became Target Cities. In addition, the differences between Target and non-Target cities may be confounded by other differences that exist between these two groups of cities.

Analysis of Covariance was used to examine possible effects of these problems on the observed relationship. ANCOVA allowed introducing controls for the baseline level and rate of violent crimes, as well as change in violent crime. It also was used to control for alternative confounding variables. Table 2 presents the results of the Analysis of Covariance. The column of "unadjusted" means are simply the mean values of change in the violent crime rate for Target and non-Target Cities before anything else is controlled. The column of "adjusted" means are the average values of change in the violent crime rate after the effects of baseline value, prior change, and city population have been removed.

The unadjusted and the adjusted results are not the same. After controlling for confounding influences, the apparent difference between Target Cities and non-Target Cities disappears. The alleged exacerbating influence of Target City grants on violent crime was an artifact of confounding influences. One or more of the problems mentioned above created a spurious association.

TABLE 2
ANALYSIS OF COVARIANCE OF
CHANGE IN VIOLENT CRIME INDEX FOR TARGET CITIES 1987-1988#

	UNADJUSTED		ADJUSTED	
VARIABLES	MEAN	N	MEAN	N
Target Cities	200.24**	7	122.28	6
Not Target Cities	16.69**	204	18.52	193

* Difference is significant at .05
** Difference is significant at .01
# Adjusts for prior change in violent crime index, prior baseline level of violent crime index, and town's population in 1987.

The source of the spurious association is identified by examining the analysis of variance summary table provided by the ANCOVA. Table 3 provides this summary table. In this table the F-test for the main effect (the difference between Target and non-Target cities) is not significant (at either the .05 or even the .10 alpha level). This demonstrates that the alleged effect of Target City grants was spurious. Control variables make it disappear.

The effects of the control variables (the sources of "adjustment") are found in the table under "Covariates." The only control variable having a significant F-test is Prior Change in

Violent Crime Index (1986-1987). **This means that the source of the spurious association was the prior trend in crime change. Target cities started off with larger increases** in violent crime rates. The observed difference between Target and non-Target Cities was merely a continuation of a trend that existed prior to introduction of the Target City project. If the 1987 violent crime rate measure had been significant, the difference between Target and non-Target Cities was an artifact of the Target Cities starting off with higher crime rates. All cities with higher violent crime rates would have had larger increases in violent crime, irrespective of whether they received Target City grants or not. If the population measure had been significant, then the size of the city confounded violent crime change. Target Cities tend to be larger cities. The largeness of the cities would have produced the big increases in crime rates, rather than the Target City grants per se.

TABLE 3

ANALYSIS OF VARIANCE TABLE FOR DECOMPOSITION OF VIOLENT CRIME INCREASES

EFFECT	SUM OF SQUARES	DF	MEAN SQUARES	F	SIGNIF
MAIN					
Target City grant status	42834.2	1	42834.2	0.9	.170
COVARIATES					
Change in Violent Crime 87-86	989823.5	1	989823.5	43.8	.000
Level of Violent Crime 87	4718.8	1	4718.8	0.2	.648
Town Population 87	9845.2	1	9845.2	0.4	.510
TOTAL					
Effect of All Influences	1081423.2	4	270355.8	12.0	.000
Residual (unexplained)	4379196.7	194	22573.2		

Just as the "significant" difference between the Target and non-Target cities was an artifact, the non-significant difference between Task Force cities and non-Task Force cities may also be a false finding. A "suppressor" variable produces the false negative result. It suppresses (or hides) the true difference.

Another candidate as a suppressor is whether or not these cities also had major changes in their management information systems. Massachusetts has over the last several years made grants to local police departments to aid them in the purchases of computers, software, programming assistance, and training. Controlling for whether a town received such a grant may change the difference between Task Force cities and non-Task Force cities.

A two-way Analysis of Variance was used to examine for suppression effects of the computer grants. Cities were coded 1 if they received a computer grant, 0 if they did not. The results are in Table 4. A significant difference between Task Force

cities and non-Task Force cities occurred after the confounding effects of the computer grants is removed.

TABLE 4
TASK FORCES AND VIOLENT CRIME CHANGE CONTROLLING FOR COMPUTER GRANTS

TASK FORCE STATUS	UNADJUSTED MEAN	N	ADJUSTED MEAN	N
Not Task Force City	19.33	103	19.99*	103
Task Force City	2.47	108	1.84*	108

*Difference significant at .05.

The positive values in Table 4 indicate that violent crimes increased between 1987 and 1988 in both Task Force cities and non-Task Force cities. However, the increase in Task Force cities was substantially smaller than in non-Task Force Cities. After controlling for computer grants, the increase in non-Task Forces was nearly eight times larger than in Task Force cities. If this finding held up to further analysis, it could be said that the Task Forces reduced the rate of increase in violent crime.

The analysis of variance also reveals a possible interaction effect between the computer grants and task forces. The F-test for the 2-way interaction between these two variables was highly significant. Table 5 presents the mean violence increases for combinations of Task Force and computer grant status. The difference between Task Force cities and non-Task Force Cities is much greater for those cities receiving computer grants. Among cities without computer grants, the violent crime increase is five times smaller in Task Force cities than in non-Task Force cities. The gain in violence is twenty-five times smaller in Task Force cities with computer grants than in those without grants. The presence of computerization appears to strengthen the efforts for the drug Task Forces.

TABLE 5
VIOLENT CRIME CHANGE BY TASK FORCE AND COMPUTER GRANT STATUS

GROUP	MEAN	STD DEV	N
No Computer Grants			
no Task Force	10.7	48.0	93
Task Force	2.1	58.8	94
Computer Grants			
no Task Force	99.8	161.1	10
Task Force	4.7	14.8	14

The additional analysis required to establish these findings regarding suppression and interactive affects is beyond the scope of this paper. What matters in this instance is that without controlling for the problems mentioned above, the possible suppression effect and the interaction effect of the computer grants would not have been discovered; and one may have reached the false conclusion that Task Forces had no effect on violent crime increases.

SUMMARY

This paper has demonstrated three important issues when analyzing change data to assess drug strategy impacts: trends in change measures, baseline values, and confounding alternative variables. The problems of trends in change data and the need to control for confounding factors were shown to be especially significant. When analyzing data for impacts, strategies such as those mentioned above need to be employed to reduce the risk of false or erroneous results.

Some consideration is needed to identifying key variables to be controlled. Possible control variables to be considered include: degree of project implementation, unemployment, poverty, level of education, divorce, population migration and age structure, city size, and proximity to major transportation routes.

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